

simple and virtuous community. Not that the author was indifferent to the natural wonders, which are so liberally scattered over the face of this wild and peculiar region; these he describes with animation and felicity; while the principal charm of his narrative arises from the fact, that it is a fireside picture.

ART. VI.—*Cholera.*

London Quarterly Review. No. XCI. Article VI. *On the Cholera.*

No apology can be necessary for calling the attention of our readers to the subject of the Cholera. The great extent of territory over which this scourge of the human race has already passed, the violence and fatality of its character, its total disregard of climate, the uncertainty as to the mode by which it is propagated, and above all, the well-grounded apprehension, that it may yet reach our continent, make it a topic of deep and fearful interest to the whole community. We feel it to be our imperative duty to contribute our aid to enlighten the public mind, and to allay, in some measure, the alarm that has been excited on this subject; and we know of no way of doing this so effectually, as by examining the mode in which the disease is propagated, or in other words, discussing the question, whether it be contagious or not. This can be done in a manner perfectly intelligible to all persons, merely by discarding the professional language, which is commonly used in this discussion, but which is by no means essential to the perfect understanding of the subject.

It is our intention, then, to confine ourselves to the examination of the question of contagion, leaving all the points connected with the history, symptoms, and mode of treatment of the disease, to professional works. These have all been ably treated in the numerous publications which this pestilence has brought to light, and they are the very topics on which those at a distance are the least able to give an opinion; while their situation, remote from the controversies that have been going on, enables them to examine the evidence as to the contagion of the disease with more fairness and impartiality, than those whose personal feelings have become enlisted on either side of the question.

As the article in the ninety-first number of the London Quarterly Review, on the subject of Cholera, has been extensively circulated in this country, and has produced a great influence on the public mind, and as it embodies nearly all the evidence and arguments in favor of the contagious character of the disease, we shall examine with some attention the reasons which the writer of it assigns for his opinion, and such others as we have seen in other writers, point out as far as we are able their fallacy, and then bring forward the evidence, which, to our minds, is irresistible against the doctrine of contagion.

It may be well to premise, that the terms *contagion* and *infection* are now often used in a very loose sense, one of them frequently in a different one from that which its etymology would indicate. By most writers, as in the article in the Quarterly Review, they are employed as synonymous terms. The term *contagious* (from *contingo*) was originally applied to those diseases only, which were communicated by *contact* with the sick, as the plague, itch, &c.; while that of *infectious* (from *inficio*) was employed to designate those which arose from any noxious matter, whether proceeding from a diseased animal body, or any other source; so that all contagious diseases were embraced under the term infectious, though all infectious ones were by no means included under that of contagious. But the term contagion is not now used in this restricted sense, either by medical or popular writers. All diseases are at the present day called contagious, which can be communicated from the sick to the well, without regard to contact; and it is not uncommon to use the term infectious as synonymous with it, though it is also employed to denote those diseases, which arise in certain seasons and climates from noxious exhalations. The fact then seems to be, that the term contagion has a more extended meaning than formerly, and we shall accordingly use it to denote that property of a disease, which enables it to communicate the same disease to those in health, either immediately or mediately, that is, either by contact or by imparting the contagious principle to the air or other inanimate substances.

The Cholera had occasionally appeared for many years in various parts of India, but it did not, till the year 1817, assume the epidemic and fatal character, which it has since exhibited in so remarkable a degree. In that year, it is admitted by all

writers that it broke out simultaneously in different parts of the province of Bengal, appearing as an endemic, dependent on the state of the soil, climate, season, or atmosphere, and that it was not owing to contagion. This is conceded by Scott, Kennedy, and others, who contend for its contagious character, as well as by those who deny it. But the two parties differ entirely as to the mode in which it was afterwards propagated; one contending that it is by contagion, while the other insists, that it is dependent on a peculiar state of the atmosphere, not cognizable by our senses. We shall not notice the opinion of those, who attribute it to a combination of these two causes, as it is unphilosophical to assign two causes for an effect, when one is sufficient to account for it; nor that which considers that it may be owing to some noxious exhalations from the earth, for it has raged with great violence, particularly at Orenburgh, during the severity of winter, and when the earth was covered with snow.

In India, the opposers of the doctrine of contagion were by far the most numerous. It is remarked by Mr. William Scott, the author of the Madras Report, and a decided advocate of the contagiousness of the disease, that 'if this question could have been decided simply by the opinions of a majority of medical men, it would have been already set at rest *against the doctrine of contagion* or infection; for there are few subjects, perhaps, on which so little diversity of sentiment has existed.' But as questions of this sort cannot be settled in this way, let us look a little at the reasons assigned by the advocates of contagion for their opinion.

One of the reasons assigned in the Review is, that the Cholera, in its progress both in Asia and Europe, has passed along 'the great thoroughfares of the country,' through which it has travelled. This no doubt is true in part, but it has not been confined to those thoroughfares, nor does it follow all of them. It appeared at Calcutta as early as August, 1817, and though there was constant intercourse by water with Madras, it did not reach the latter place till October, 1818, travelling at the rate of little more than two miles a day, and visiting all the intermediate places. Now does this look like the course of a contagious disease? Does it not look rather like the progress of some atmospheric poison? If it had been propagated by contagion, is it not probable, as there were no quarantines, no interruption of intercourse with the sick, that some, who had

become infected would have left the city immediately after, before they were aware of it, and sickened with the disease at a distance? Is it not probable, that it would have appeared on board of some of the country trading ships bound to Madras, and thus have been conveyed to that city in a few weeks or months at the farthest, if it could have been so conveyed, instead of passing over land at a regular and slow rate of progress, and not reaching Madras till more than a year had elapsed?

It is well known that the Cholera spread in various directions from the province of Bengal, travelling towards China in one direction, across the Delta of the Ganges in another, and extending on the south and east nearly to New Holland. If it were propagated by contagion, why did it stop there? Why did it not extend itself over the vast continent of New Holland, passing along the great thoroughfares of the country? There were routes of human intercourse in that direction, which it might traverse, and human beings enough for victims.

It is said with great confidence in the *Quarterly Review*, that 'whenever it invades a new country, it begins in a great commercial mart. There seems to be no exception to this law, except where the disease has been imported by invading armies.' The Cholera first broke out in Jessure in the interior, about sixty miles from Calcutta, in 1817, and it travelled over the peninsula of India in 1818, at the rate of about one degree a month, and did not reach the seaport of Madras, a great commercial mart, till it appeared simultaneously in parallel latitudes in the interior, though 'some of the many trading vessels must have carried it speedily from the tainted districts to the seat of the Presidency, had the disease been capable of being conveyed by man or merchandise.*' It reached Madras on the 8th of October, 1818, and on the 10th of that month, the port is annually closed for two months, in consequence of the surf and prevailing winds, and the small trading vessels are drawn up on land. Notwithstanding this interruption of human intercourse, this singular disease travelled on over the next five degrees of latitude, 'even more rapidly than over the former six,' and arrived at Cape Comorin by the 1st of January, 1819.

The facts just stated, and about which all the writers on the disease in the East are agreed, are a satisfactory refutation of

* Bell on the Cholera, page 80.

another assertion of the *Quarterly Review*, which is, that the Cholera 'does not attack a large space of territory of a new country at once, but gradually; the first point of attack being invariably on a frontier or coast.' This certainly was not the case in its progress in India; it appeared simultaneously in the interior and on the coast, and extended over a large tract of country at once.

Again, when it appeared on the Persian Gulf, it attacked several places remote from each other, simultaneously, passing over a great extent of territory. Yet the assertion we have just quoted from the *Review* is given as if it were a well known fact, universally admitted, in relation to the progress of the disease.

The reviewer states, as an additional argument in favor of the contagion of Cholera, 'that the rapidity of the propagation of the disease appears to have been proportional to the distances, and to the means of communication.' If the previous history of Cholera in Asia and the continent of Europe were not sufficient to satisfy him of the error of this statement, he must have perceived it before this time by what has occurred in the island of Great Britain. Three months nearly elapsed after it broke out in Sunderland, before it appeared in London, notwithstanding the shortness of the distance, the facility and frequency of the communication, and the absence of all quarantine on those who travelled by land.

Another reason, and perhaps the strongest that has ever been brought forward on that side of the question, and which is often urged in favor of the contagious character of Cholera is, that it extends itself in defiance of climate and season, and spreads as well in the cold regions of Russia as under the burning sun of the East, and regards neither the frosts of winter nor the heats of summer. It is not, perhaps, strictly true that it is wholly uninfluenced by season; it is supposed to have been checked between Arabia and Syria in 1821, and at Astrakhan in 1823, by the approach of winter; but it cannot be denied, that, though cold may retard its progress, it does not destroy the disease, and in some instances, as at Orenburgh, it does not seem to check it.

But does it differ in this independence of climate and season from other epidemics, that have been propagated by atmospheric influence?

'The Influenza of 1781 and 1782 is said to have originated

in China, and to have travelled through Asia into Europe ; where it crossed the Atlantic, and arrived the ensuing year in America.*

The Influenza of 1815 originated also in China and spread throughout Asia, Europe and this country. It has been stated in regard to the epidemic of that year, that the crews of several vessels on the Atlantic became affected with the disease at sea, in consequence of having fallen in with the current of air, which was bearing the germs of the Influenza across the Atlantic.

In the second volume of Freind's History of Physic may be found a short account of an epidemic that originated in Asia, about the year 1345, 'and from thence travelled over all the world, and destroyed a *fourth* part of mankind ; in the East it lasted three years, and was more mortal.' Webster, in speaking of this pestilence, says, that the facts connected with it annihilate, at a blow, the whole doctrine of the propagation of that disease from country to country by infection.

The most remarkable epidemic, however, of which we can find any record, first appeared in the year 540. We extract the following account of it from Webster's History of Epidemic Diseases, who transcribed the particulars, he says, from Procopius and Evagrius, two contemporary historians ;

'Procopius relates, that this pestilence, which almost destroyed the human race, and for which no cause could be assigned but the will of God, did not rage in one part of the world only, nor in one season of the year. It ravaged the whole world, seizing all descriptions of people, without regard to different constitutions, habits, or ages ; and without regard to their places of residence, their modes of subsistence, or their different pursuits. Some were seized in winter, some in summer, others in other seasons of the year.

'It first appeared in Pelusium, in Egypt, and thence spread westward to Alexandria, and all parts of Egypt ; eastward towards Palestine, and extended to all parts of the world,—laying waste islands, caves, mountains, and all places where man dwelt. If it passed by a particular country at first, or *slightly affected it*, it soon returned upon it with the same desolating rage which other places had experienced. It began in maritime towns, and spread to the interior country.

'Neither physician nor attendant caught the distemper by contact of the sick or dead ; and many, encouraged by their wonderful escape, applied themselves with assiduity to the care of the sick and the burial of the deceased.'

* Hawkins on Cholera, page 208.

We will make one short extract only from Evagrius.

‘But what, above all, appeared singular and surprising was, that the inhabitants of infected places, removing their residence to places where the disease had not appeared, or did not prevail, were the only persons who fell victims to the plague in the cities which were not infected.’

The duration of this pestilence has been stated to have been fifty-two years, though Webster thinks that there was a series of severe epidemics during that period, and not a single epidemic only.

We have presented this account in the language of the writers themselves, lest we should be suspected of having given a coloring to it favorable to our own views. With the slightest attention, it will be seen how much this pestilence resembled the Cholera in its progress. It regarded neither climate, nor season, nor situation; if it passed by a place, it afterwards returned to attack it, as Cholera is known to do; and it began in maritime towns, as it has been said that Cholera does, and thence spread into the interior; and yet this was admitted to be an atmospheric disease, not communicated by contact of the sick or the dead.

We have now completed our examination of what the reviewer calls his first class of evidence, ‘resting solely on those facts concerning the rise and progress of the malady which are admitted by every one,’ and shall next pass in review his second class, or the direct evidence of the contagiousness of the disease. He divides this latter evidence into three kinds. The first proves the contagious character of the disease *positively*, as when it has been propagated by the known intercourse of the uninfected with the infected; the second proves it *negatively*, as when it is shown that they who avoid intercourse with the sick escape the malady, though living under the same general circumstances of climate, food, &c.; and the third includes what are called facts of coincidence, as when the disease breaks out in a healthy place, after the arrival, from infected places, of individuals not laboring under the malady. On this last order of facts, mainly depends the evidence in favor of the propagation of the disease by merchandise and other inanimate substances.

1. Under the first head, the reviewer adduces several facts to show that the Cholera broke out in places through which armies had marched, or that it attacked a detachment of troops before

uninfected, on joining another in which the disease existed. That our readers may be the better able to judge of the value of these facts, we must ask their attention to the following cases.

In November, 1817, the Cholera broke out in the great Indian army, under the command of the Marquis of Hastings, consisting of ten thousand troops and eighty thousand followers, then concentrated near the banks of the Sinde, in Bundelkund. Such was the violence of the disease, that in a little more than twelve days, nearly nine thousand persons fell victims to it. The commander, perceiving that the plans of his expedition would be frustrated, if the whole army were not destroyed by the disease, determined to change his place of encampment. Though compelled to leave many of the sick behind, he carried many with him, and at length pitched his tents fifty miles from his former position, on a dry and elevated spot; 'on the 19th he crossed the clear stream of the Betwah, and upon its high and dry banks at Erich, he got rid of the pestilence, and met with returning health.*'

Another case equally striking may be found in the same Report. An immense concourse of people, believed to be between one and two millions, were assembled on the banks of the Ganges in the month of April, to celebrate a religious festival.

'It is the custom of the pilgrims to repair to the bank of the river, where they pass the night with little, if any shelter; many persons being crowded together under the cover of a single blanket, thrown out as an awning. The temperature is very variable; the days being hot and the nights cold, with heavy dews, and sudden chilly blasts from clefts in the mountains.

'On the present occasion, these causes were sufficient to generate the Cholera; which broke out soon after the commencement of the ceremonies, and raged with such fury, that in less than eight days, it is said to have cut off above twenty thousand victims. But so confined was its influence, that it did not reach the village of Juwalapore, only seven miles distant; and ceased immediately on the concourse breaking up on the last day of the festival.'

At the one hundred and thirty third page of the work from which we have just quoted, may be found an account of the disease prevailing to a great extent in a detachment of troops, yet, on joining another body only five miles distant, though the men of this party, who had been exposed to the disease, mixed

* Bengal Report, page 16.

promiscuously with those of the Sauger troops, yet of the latter, not one individual got the disease.

In a note in the two hundred and forty second page of Annesley's work on the diseases of India, is the following statement.

'Cholera attacked the field force stationed at Malligaum in Kandiesh, and raged with great violence amongst the corps posted on the left of the line; while the seventeenth battalion of native infantry, who were posted on the right of the line, were exempt from it, notwithstanding they had continued communication with the other men. But although they were exempt from the disease while they remained in this position, they suffered very much from Cholera on their march from Malligaum to join Major-General Sir John Doveton's force in the Ellichapoor valley.'

The latter part of this statement is highly important, as it shows that the exemption of the men did not arise from the absence of pre-disposition for the disease.

The information contained in the following extract from Mr. George H. Bell's admirable work on Cholera is so valuable, that we give it at length and in his own words.

'In July, 1819, I marched from Madras in medical charge of a large party of young officers, who had just arrived in India, and who were on their way to join regiments in the interior of the country. There was also a detachment of sepoys, and the usual numerous attendants and camp-followers of such a party in India. The Cholera prevailed in Madras when we left it. Until the fifth day's march, (fifty miles from Madras) no cases of the disease occurred. On that day several of the party were attacked on the line of march; and, during the next three stages, we continued to have additional cases. Cholera prevailed in the country through which we were passing. In consultation with the commanding officer of the detachment, it was determined that we should endeavor to leave the disease behind us; and as we were informed that the country behind the Ghauts was free of it, we marched without a halt, until we reached the high table land of Mysore. The consequence was, that we left the disease at Vallore, eighty-seven miles from Madras, and we had none of it until we had marched seventy miles farther, (seven stages) when we again found it at one of our appointed places of encampment. But our camp was, in consequence, pushed on a few miles, and only one case, a fatal one, occurred in the detachment. The man was attacked on the line of march. We again left the disease, and were free from it during the next hundred and fifteen miles of travelling. We then had it during three stages, and

found many villages deserted. We once more left it, and reached our journey's end, two hundred and sixty miles farther, without again meeting it. Thus, in a journey of five hundred and sixty miles, this detachment was exposed to, and left the disease behind it, four different times; and on none of these occasions did a single case occur beyond the tainted spots.' Pages 90, 91.

It appears, that in the four first cases just cited, large bodies of men, part of whom were then laboring under Cholera, and all of whom had been exposed to its atmosphere, on going into other districts and associating with those in health, did not in a single instance convey the disease to other persons; and in the extract from Mr. Bell, we see that men in health, while passing through an infected district, without any communication with any human beings who had the disease, became the subjects of it, and yet were unable to communicate it to others; and that the disease ceased as soon as they had passed through the infected district, and appeared again on entering another.

In stating that Cholera has frequently broken out during the march of troops in India, or when one detachment has joined another, as evidence of contagion, the writer seems to have forgotten the fact, that the exciting cause of the disease, whatever it may be, was every where lurking about in that country, waiting only for the pre-disposing causes to enable it to attack. Nothing is more likely to produce this pre-disposition, than the fatigue and exhaustion consequent on such marches in such a climate.

The occurrence of the disease in villages through which troops have passed, or its appearance in one detachment, which had been previously exempt, when another had joined it, ought to be considered, when viewed in connexion with the strong facts on the other side, as a coincidence that might occur in India at any time since Cholera was first epidemic there, or in any country where the disease prevailed. At any rate, it cannot be considered as *positive* evidence of contagion.

2d. We come now to the reviewer's second division of facts, which, according to him, prove the contagiousness of the disease *negatively*. These consist of statements of cases of individuals, who, in places where Cholera has prevailed, have escaped by insulating themselves and cutting off all communication with other parts of the infected district. This course was adopted by the French Consul, when the disease was at Aleppo. He retired with two hundred other persons to his

country seat, at some distance from the city, and they all escaped the epidemic by a rigid quarantine. 'The large establishment of military cadets at Moscow was preserved by a similar plan, from a scourge which was so active on all sides.' Whole towns are said to have escaped by adopting the same means.

We admit the facts stated above, but we by no means assent to the reason assigned for this exemption from the disease. Even the advocates of contagion acknowledge, that those only are attacked with Cholera who have a strong pre-disposition for it, and they place among the principal pre-disposing causes, intemperance and excess of all kinds, deficiency of food, food of bad quality, the debilitating passions of the mind, and excessive fatigue. Is it probable that persons thus secluded would labor under any of the pre-disposing causes? They are, for the most part, persons in health, of ample means to furnish themselves with all the comforts of life, and at the same time, aware of the danger of indulgence. It could not be expected that such persons would be attacked, unless the affecting cause were more virulent than ordinary.

But a perfect answer is, that all these precautionary measures have not unfrequently been unavailing, and that the disease has broken out in towns subjected to the most rigid quarantine, and in places perfectly insulated. This was the case at Thorn, as appears from the following extract of a letter from the British Minister at Berlin to Lord Palmerston, dated July 26th, 1831.

'My Lord,

'The Cholera has broken out at Thorn, notwithstanding the strict measures of precaution adopted there.'

'Signed, G. W. CHAD.'

The case of Egypt may be cited as an example of the same thing on a larger scale. The Quarterly Review, which contained the statement that this country had escaped the Cholera in consequence of the vigor of her quarantine system, was hardly out of the press, before the news arrived in England, that the disease had broken out in Egypt. In the London Medical Gazette of Jan. 14, 1832, will be found an extract from a letter announcing this fact.

It entered Prussia, notwithstanding all the efforts made to exclude it. In the proclamation of the King, dated Sept. 6th, 1831, he says, that the 'Asiatic Cholera had penetrated into

his dominions, in spite of measures the most vigorous, precautions the most active, and vigilance the most sustained, which had all proved useless and unsuccessful in averting or even checking its progress.'

Dr. Jaehnichen informs us, that the complete insulation of some persons, and even whole families, during the prevalence of the Cholera at Moscow, did not always preserve them from it.*

Mr. Scott, in the Madras Report, states, that 'at Masulipatam, a town on the Coromandel coast, the disease first appeared among the convicts confined in the fort, and that it was not till about ten days afterwards,—July 10, 1818,—that it was observed in the town and neighborhood.'

In the Edinburgh Medical and Surgical Journal for Oct. 1831, will be found, in some observations on Cholera, by Dr. H. L. Gibbs, of St. Petersburg, an account of a patient who was attacked with Cholera at the Naval Hospital and died of it. 'From the great precaution used in avoiding communication, this man, who was confined to his bed in the hospital, must have been affected, I think, by pre-disposition idiopathically;' and it appears that no other person, either before or after, had the disease in the Hospital.

The exemption from Cholera of some who have secluded themselves, while it was raging in this vicinity, does not prove that it is propagated by contagion, while its attack of others who have been equally secluded, shows that it can be, sometimes at least, propagated by other means, and this is sufficient for our purpose.

3d. We come now to the third division, the *facts of coincidence*, as when the Cholera breaks out in a healthy place after the arrival, from infected places, of individuals who do not themselves labor under the malady. On these facts depends the evidence in favor of infection by merchandise or other inanimate substances. We shall examine two of the most important facts which the reviewer brings forward, merely remarking, that if we can prove, as we think we shall be able to do, that the disease cannot be propagated by inanimate substances, the whole of this division falls to the ground.

The first of these facts is the supposed introduction of the Cholera into the Mauritius from Ceylon, by the Topaz frigate. The disease reached Ceylon in 1818, but it did not appear in the Isle of France till 1819, at which time the reviewer says

* Page 24 of his work on the Cholera.

it was carried there by the frigate above named. In answer to this we will give an extract from an article in the *Asiatic Journal*, on the Cholera, in which, by the way, the doctrine of contagion is maintained.

‘The disease appeared extensively in the Island in November, 1819, and has been supposed to have been brought thither from Ceylon, by the *Topaz* frigate, which arrived at the Mauritius in October. But a careful inquiry into the circumstances of the case convinced a committee of British medical officers, that the disease was not imported, nor of foreign growth. In their report, dated 4th of December, they state, that the first case occurred so early as the 6th of September, and “that they feel the strongest persuasion that it is not of a contagious nature, and that it is not of foreign introduction.” In these two conclusions the French medical gentlemen unanimously concurred, and both considered the disorder as promoted, if not produced, by the great and sudden vicissitudes in the temperature. The report adds, that a similar epidemic prevailed in the colony for some time in the year 1775.’

Admitting, what we are by no means disposed to do, that the statement of the medical gentlemen of the Island is incorrect, it is not pretended by the reviewer that there were any cases of the disease on board the frigate at the time of her arrival, or that any cases appeared there before the 18th of November, three weeks after, a period rather too long to suppose it possible that there could be any connexion between them and the ship. Even the British Board of Health, all firm believers in contagion, have fixed the ultimate period, which elapses between exposure to the cause and the appearance of the disease, at five days. It is, perhaps, proper to observe, that the reviewer says that the disease was propagated in Mauritius by goods or inanimate substances, which were carried there by the *Topaz*. We shall see, in speaking of the Cholera at Warsaw, on what foundation this opinion rests.

From the Isle of France, he states that it was conveyed to the Island of Bourbon, and speaks with some degree of triumph of the means that were adopted there to arrest its progress. His facts are all derived from Mr. Kennedy’s work, for which we cannot be suspected of having any very strong partiality, when we state, that he is so decided an advocate of contagion, that he will not consent to call the disease by any other name than that of the *contagious Cholera*. We will therefore make the following extract from his book, and then offer a few comments.

‘ Taking advantage of the terrible example afforded in Mauritius, the governor of Bourbon, a neighboring Island, distant about two degrees, adopted sanitary precaution to exclude the contagion. On the 7th of January, however, a vessel called the Pic-Var, from Port Louis, arrived off Bourbon, and had intercourse with the shore. The Cholera broke out seven days afterwards, in the town of St. Denis. Nothing dismayed by this unfortunate circumstance, the governor ordered cordons of troops to be posted to cut off all communication with St. Denis, the focus of the malady, and a lazaretto was established for the reception of such persons as might be attacked. Cordons were also established for their preservation at St. Susanne, St. André, and St. Benoit; but in the consternation which seized the inhabitants of these parishes, they dispersed, to seek safety in the interior of the country. The alarm created by the pestilence in Bourbon, and the vigorous proceedings of the governor, Baron de Mylius, may be conceived from the concluding sentence of the Order of the Day, which was, *surveillance ou la mort*. The consequences of these measures corresponded to the decision with which they were carried into effect. The Cholera did not extend in Bourbon, as it had done in Mauritius, and the whole number of the persons attacked scarcely amounted to a few hundreds.’ Page 204.

Admitting all the facts to be as he has stated them, it will be seen in the first place, that there was no evidence that the Cholera was on board the vessel that arrived off Bourbon; in the second place, that the Cholera did not appear till seven days after she had had intercourse with the shore; and Mr. Kennedy, in another part of his work, lays it down as one of the laws of Cholera, that ‘ the period of time during which the contagion lies dormant in the system rarely exceeds three days; and lastly it appears, that though a cordon was established, to prevent the intercourse between those who had been exposed to the sick and the other inhabitants of the Island, yet all those who had been thus exposed, broke through the cordon and sought safety in the interior of the country. And what was the consequence? No one was attacked with the disease who went into the country, no one communicated it to another, and ‘ the Cholera did not extend in Bourbon, as it had done in Mauritius.’ A stronger case against contagion can hardly be imagined than this.

The second case referred to in the Review, is the appearance of the disease at Warsaw. Let us look at the facts of the case. The battle of Iganie, between the Poles and the Russians, took place on the 10th of April, 1831, and on the night of

the 12th and 13th, twelve Polish soldiers were attacked with Cholera, which soon extended to others, both in Praga and Warsaw, situated on the opposite sides of the Vistula. It was at once asserted, that they contracted the disease from the Russian army. Is this probable? Is it possible that any man of that army could have gone into battle, while laboring under the Cholera? and if not, the Poles were of course not exposed to any *persons* who had the disease; and it must have been taken, if taken at all from the Russians, from the contagious matter adhering to their clothes, or other inanimate substances. This, in fact, seems to be the opinion of the reviewer.

We would remark in the first place, that it is very improbable that a contagious principle sufficiently powerful to affect others, could be carried about by individuals in their clothes, without affecting themselves. And in the second place, most of the contagionists are now of opinion, that the disease cannot be communicated by inanimate substances. In the official reports made to the British Government, by Drs. Russell and Barry, will be found a paper from Dr. Doepp, Director of the Foundling Hospital at St. Petersburg, containing the following statement.

‘I am of opinion that the exhalations of the sick are the carriers of the disease, but only so long as they retain their vaporous form. I have given myself great trouble to ascertain if the clothes and linen covered with the perspiration of the sick were capable of transmitting the contagion; but I could not meet with any instance of it. Children taken from the cold, clammy breast of the mother, or wet-nurse, and given over to another nurse to suckle, did not infect the latter. This occurred in my presence.’

Dr. Albers, in his report to the Prussian Government, concludes with the following sentence.

‘I have met with no instance which could render it at all probable, that the Cholera is disseminated by inanimate objects.’

In an official report of the British Central Board of Health, all the members of which are contagionists, dated Jan. 4th, 1831, are some statements, which show the extreme improbability that the disease is ever propagated by inanimate substances.

‘There is perhaps,’ say they, ‘no question in the whole range of sanitary police, in which so many and such irrefragable facts can be brought to bear as on this; derived, too, from the most

authentic and recent sources. Seven hundred and thirty-two ships, loaded with hemp and flax from infected ports of the Baltic, arrived at the different quarantine stations in this country between the 1st of June, and the 31st of December, 1831. Many vessels also arrived laden with wool and hides, yet not a single case of Cholera occurred on board any of these ships outside the Cattigate sea, nor amongst the people employed in opening and airing their cargoes in the lazarets.

‘At the hemp and flax wharves at St. Petersburg, where several thousand tons of these articles arrived during the spring and summer of this year, from places in the interior where Cholera existed at the time of their departure for the capital, the persons employed in bracking or sorting, and who generally passed the night amongst the bales, did not suffer so early in the season, nor so severely, as other classes of the general population.’

The evidence on this point, that the disease cannot be conveyed by merchandise, or other inanimate substances, is now considered by most, if not all the contagionists, as conclusive; otherwise we should produce much more to corroborate our opinion. This we think renders it clear, then, that the disease was not introduced into Poland in this way.

There really seems to be no difficulty, in accounting for the appearance of Cholera at Warsaw, on the supposition that it was an epidemic, propagated by the atmosphere. It might very justly be said, that the epidemic constitution of the air, which produces this disease, had reached Poland, and would attack those who were strongly pre-disposed to it. What stronger pre-disposition could be imagined, than the one which we know the Polish soldiers at that time possessed? We translate the following passage from the work of M. Brierre-de-Boismont, an intelligent French physician, and a believer in contagion, who went from Paris to examine the disease in Poland, and has since published an account of it.

‘If we imagine,’ says he, ‘thousands of men, pale, haggard, sallow and emaciated, whose features denoted suffering, weakened by long marches and privations of every kind, bivouacking for five months of extreme cold, in the woods or on the ground, which was almost always marshy, we shall still have but an imperfect idea of the condition of these miserable victims of the war.’

When we consider, too, the excitement of the battle, and the fatigue and exhaustion consequent on it, there seems hardly a pre-disposing cause of the Cholera, which did not operate upon them.

We have thus noticed the facts and arguments brought forward by the reviewer, in favor of the contagion of Cholera; but as much light has been thrown on this subject by the progress of the disease during the last fifteen years, we must trespass a little longer on the patience of our readers.

It will be recollected, that this disease first appeared as an extensive epidemic, in the province of Bengal, in the summer of 1817. It reached Bombay in August, 1818, and from this place, the reviewer states that it was carried to the Arabian town of Muscat, about 300 leagues distant, situated at the mouth of the Persian Gulf. He takes some pains to show the great commercial relations subsisting between Bombay and the ports on the Persian Gulf as early as the year 1818, stating that there were at that time 120 ships, employing 1000 hands, besides '730 country ships, which, belonging to the various ports of the western coast of India, often touched at Muscat in their voyages to more distant lands.' The intercourse between Bombay and Muscat is no doubt great, and no quarantine was at any time imposed on the vessels, or on their cargoes, crews, or passengers, so that if the disease could have been conveyed in this way, it would no doubt have soon been done. But what was the fact? The Cholera did not appear in Muscat till June, 1821, nearly three years after it attacked Bombay, and no one has pretended to point out the ship that carried it there.

It appeared on both sides of the Persian Gulf, extending over a considerable part of Arabia and Persia. 'Bassora, which is situated at the head of this Gulf, on the river Euphrates, was attacked,' says the reviewer, 'nearly at the same time as Muscat,' though it is ten degrees to the northward of it. Moreau-de-Jonnès, the oracle of the contagionists, puts down the appearance of the disease in the two places in the same month, and this took place before it had visited the intermediate country. The simultaneous appearance of the disease, in places so remote from each other, it is wholly out of our power to explain on the principle of contagion, though the difficulty vanishes if we suppose the seeds of the pestilence to be conveyed by the atmosphere.

It reached the desert which separates Arabia and Syria, in the autumn of 1821, and ceased on the approach of winter, but re-appeared without any assignable cause, in the following spring, in the neighborhood of the Tigris and Euphrates, and arrived on the shores of the Mediterranean in August, 1823.

‘Once established,’ says the reviewer, ‘on the shores of the Mediterranean, every facility to its immediate transmission into European ports appeared to be offered;’ and it no doubt would have been transmitted there, if it were possible thus to have conveyed it. No quarantines were laid, no restrictions of any kind were imposed, the great ‘thoroughfares of the country’ were thronged with human beings, but the disease did not follow in their train, and it ceased spontaneously at Tripoli in Syria, having attacked, it is said, only thirty-one persons, out of a population of fifteen thousand.

On the other side of the Gulf, it extended through Persia, and finally reached Astrakhan, at the mouth of the Wolga, on the Caspian Sea, in the autumn of 1823. Here again the disease seemed to threaten Europe, and it would no doubt have been conveyed throughout the vast territories of Russia at that time, by passing up the Wolga, one of the great thoroughfares of the country, if it had depended on contagion for its propagation. But, as was before remarked, it subsided at the approach of winter, and finally disappeared.

During the six succeeding years, no alarm was excited by it in any portion of Europe. It appeared, however, in various parts of Persia, China, and other countries of the East, and raged at times with great violence. Towards the close of 1826, it broke out in Mongolia, and reached almost to the borders of Siberia.

In August, 1829, it appeared at Orenburg, the capital of the Russian province of that name, and continued for about three months in the city, and till February, 1830, in other parts of the province. It has been asserted, on the authority of M. de Jonnès, that the disease was carried there by the caravans, who bring across the steppes of Boukara the merchandise of China, Thibet, Caboul, and Hindostan. It might be enough to say, in answer to this, that the official reports of the Russian Government admit that there is no evidence of the fact. Professor Lichtenstædt, after a most careful investigation of the subject, acknowledges the same thing; and the editors of the Edinburgh Medical and Surgical Journal, who believe in the contagious character of the disease, say that there is no reason to suppose that the Cholera was brought from the East or elsewhere to Orenburg. This, in ordinary cases, might be thought conclusive; but, as we wish to show how much credit is to be attached to the statements of M. de Jonnès, who first asserted

that the disease was carried by the caravans, and who is the great source whence the Quarterly reviewer derives his facts in favor of contagion, we shall give the following extract from the thirty-second volume of the London Asiatic Journal.

M. Moreau de Jonnés read a paper before the French Academy of Sciences, November 22, 1830, on the progress of the Cholera. Among other things, he stated that the disease was carried to Orenburg by the caravans from the East.

‘ M. de Humboldt, who was present when the paper was read, remarked, that it appeared improbable that the *Cholera morbus* was carried by the caravans to Orenburg. In fact, when he left this place, the disease did not prevail there, yet nearly four months had elapsed since the arrival of the caravans. The plains of the Kirgheez, which these caravans had traversed, were uninfected by the disease. It was not impossible that the *Cholera* might have been brought from Samarkand by individual travellers; but this idea had never occurred to any person at Orenburg, where the disease was considered to have been generated and developed fortuitously, under atmospheric influence, and not to have been imported.’

In July, 1830, the disease appeared again in Astrakhan, and it is asserted by the reviewer, that it was brought by a vessel from Baku, a town situated about three hundred and fifty miles from Astrakhan down the Caspian; several of the crew having died on the voyage of the Cholera. A sufficient answer to this is, that it is not pretended in the Russian official report, as published by Dr. Lichtenstædt, that the vessel ever went up to Astrakhan; on the contrary, it appears, that she was put into quarantine sixty miles below, at a place called the Sedlitovski Lazaretto.

The reviewer says, ‘ once in possession of this point,’ (Astrakhan), ‘ the disease found a ready inlet to the principal towns of the Russian empire; afforded by the navigation of the Wolga, Don, and Donec, on the banks of which they are, for the most part, situated.’ He seems to have forgotten, that the disease was at Astrakhan in 1823, but did not extend at that time beyond the limits of the place, though the same ‘ great thoroughfares of the country’ were as much open then as in 1830.

In the latter part of September it reached Moscow; and Dr. Walker, a contagionist, in an official report, addressed to the British Government, dated April, 1831, observes, ‘ that a strict investigation had been made into what were reckoned the

first four cases occurring in Moscow, and that it was proved that they had neither themselves been in any infected place, nor had any communication with any one coming from such place. He farther says, 'I am convinced of the contagious nature of the disease, but the proofs of its transmission from one individual to another are not quite perfect as yet.' The Cholera continued at Moscow till February, 1831, the whole number of cases being a few more than eight thousand, and the deaths about half that number.

Of its appearance at Warsaw, we have already spoken. It broke out in Dantzic and Riga in May; Dr. Dalmas has proved that it was not imported into the former place, and the same has been shown in regard to the latter by the medical board of Riga.

The first case of the disease at St. Petersburg occurred in June. In the official reports, published by the British Board of Health, may be found a document signed by the medical and other officers of the police of St. Petersburg, acknowledging that they were unable to show 'whence the disease originated.' There is one fact in relation to this epidemic at St. Petersburg, that has been supposed to favor the doctrine of contagion; it is communicated in a letter from Drs. Russell and Barry to the British Government.

'A woman,' say they, 'had been sent out (of the city prison) some weeks before to be treated for a syphilitic complaint, in a public hospital. Her husband was also in confinement at the time, in a different part of the building, but remained. The woman was returned to jail on the 23d day of June, O. S., with a diarrhœa upon her. She saw and embraced her husband for a moment, as she passed in to be placed in the room of observation. In a few hours she was seized with true Cholera, and died that night. This was the very first case. The next persons attacked in the prison were the three women in the same room with the former, one of whom had rubbed the deceased. These three died all within three days after the first. The next prisoner attacked was the husband of No. 1; he lived in a separate part of the jail. In short, of twenty-seven attacked, (fifteen dead), there is but one to whom communication cannot be traced. He was confined for a capital offence, and had less liberty than the others.'

Now it should be considered, that at this time, the whole atmosphere of St. Petersburg, if our view of the subject be

correct, was loaded with the infection of Cholera, and it required only a pre-disposing cause to give it efficiency. The depressing affections of the mind, particularly grief and terror, are known to have a powerful influence in producing a pre-disposition for the disease; and when we add to these the kind of diet and mode of living, which criminals of this class would be likely to have, we cannot well imagine persons more pre-disposed to take the disease than those who had it. Besides, the fact, that one of the sick was confined for a capital offence, and was not known to have had any communication with the infected, affords the strongest presumptive evidence that the disease was taken from the atmosphere. It is absurd to suppose, that this last prisoner could have had communication with any person, without the knowledge of the superintendent of the establishment.

We will now finish our sketch of the progress of the Cholera. It appeared at Archangel in July, at Berlin in August, at Vienna in September, and at Hamburgh in October, in spite of the most rigorous measures of quarantine that could be adopted. It seems idle to say, that Governments, constituted like those of Russia, Prussia, and Austria, could not enforce a system of complete non-intercourse between their territories and those of the infected countries; and it is admitted by nearly all the contagionists, after the most rigid scrutiny, that there is no evidence that the disease was brought to any one of these places. We should trouble our readers with the proof of this in detail, if there were any considerable diversity of opinion on the subject.

It was thought by many, that if the disease should reach Great Britain, it would then be easy to decide the question as to its contagious character; as it was supposed to be impossible to bring it there by human agency, without the fact being known. And we confess, that we were of that number. In the first place, Great Britain was an island, separated by a considerable extent of water from every place where the disease had been; and in the second place, the Government, acting on the principle that it was contagious, had adopted a very strict system of quarantine. When, therefore, it broke out at Sunderland, as it did on the 26th of October, it was but fair to expect that the whole thing would be explained; that, if it were imported, we should be furnished with the name of the vessel that brought the unlucky patient, the place where he contracted the disease,

and what was the exposure of the first person who had it in England. We thought that every minute particular would be stated officially, that had any bearing on the breaking out of the disease; and this would no doubt have been done, as all the members of the British Board of Health are believers in contagion, if any thing had occurred which favored that doctrine. But nothing of this kind has taken place. We had, to be sure, in the newspapers a story of a vessel from Hamburgh, passing up the river by Sunderland and returning, without communicating with the shore, and she, it was said, no doubt introduced it; and then we were told, that it was brought there by a chest of clothes of a seaman, who had died of it abroad. But all these are now given up, and we are totally in the dark, if we admit the doctrine of contagion, as to the way in which the Cholera entered England.

As some may perhaps doubt the statement we have just made, we will give a short extract from a letter from Dr. J. Brown, dated Sunderland, November 10th, 1831, and which may be found in the *Medico-Chirurgical Review* for January, 1832.

‘Need I examine,’ says he, ‘the question of its importation, and refute the story circulated through the newspapers, of certain ships which lay above our bridge, and communicated the disease to the town? Those ships came from places where Cholera did not exist at the time of their departure,—most of them from Holland, where it has not yet appeared; their crews were and had been in perfect health; and the disease *first* manifested itself in a part of the town *two miles distant from where they were lying*. If there have been other modes in which disease may have been communicated from the continent, I know not of them.’

‘The importation doctrine is here,—where we must be supposed to be the most competent judges of a matter, not of opinion, but of fact,—so generally abandoned, that I shall bestow no more pains in its refutation.’

There are some, to be sure, who say, that as Sunderland is one of the nearest towns in Britain to Hamburgh, it might have been introduced by some person from that place by violating the quarantine; it will be time to consider this, when it is shown that such a violation has taken place.

The manner of its entering London, without passing through the intermediate country, though there was great and daily inter-

course between Sunderland and all parts of the kingdom, is wholly inexplicable on the principle of contagion. And is not the fact of the appearance of the Cholera in Paris still stronger against this doctrine? Here is a city, situated nearly one hundred and fifty miles from the sea-coast, and about two hundred and fifty from any place where the disease had existed, suddenly becoming affected with it. It is not pretended that the first victims of it had in any way been exposed, nor that any persons laboring under the disease had arrived there. Where then could they have become infected?

It is well known that a quarantine has been established throughout France, and any one at all acquainted with the keen vigilance of the Paris police-officers will hardly believe it possible they could have been deceived. To us the true explanation appears to be, that in the course which this wonderful epidemic is appointed to run, it had arrived at Paris, and suddenly seized on those, the state of whose systems rendered them peculiarly pre-disposed to it. This number at the present moment is greater than usual, from the embarrassment and interruption of the ordinary business of the city, and the consequent want of employment, with its attendant evils, of many of the laboring class. This will account for the fact that so many have already been destroyed by it there, and that its ravages have been chiefly among the lower orders of the people.

Having considered the principal reasons that have been urged in favor of the contagious character of Cholera, and attempted to show that the course of the disease, from its commencement to the present time, does not warrant the belief that it has been propagated by contagion, we shall present some positive evidence that it is completely a non-contagious disease.

1. The sudden disappearance of the disease in places which it has attacked, when a very small part of the population has been affected, and at a moment when great numbers are sick, and when free intercourse has been allowed with them, are facts hardly compatible with the doctrine of contagion. Two examples will be enough to illustrate this. In Moscow, with a population of between 200 and 300,000, only 8,000 were attacked, and in St. Petersburg, containing more than 300,000 inhabitants, something less than 8,000 had the disease, and it suddenly ceased at a time when a large number were sick.

2. In almost all places from which we have a right to expect authentic accounts in Europe, we find that the Cholera

has been preceded by a great tendency to derangement of the stomach and bowels among the population generally, showing that there is, what Sydenham called an epidemic constitution of the air. This was noticed in many places in Russia, Germany and Great Britain. We refer for information on this subject to the very excellent letter of Dr. Brown, of Sunderland, from which we have already quoted.

3. During the prevalence of the Cholera in a place, the brute animals have frequently been sick, and many of them have died. This is spoken of by Jameson, in the Bengal Report, as having been the case in the East Indies, and it has also been noticed in Russia, Germany and Great Britain.

4. The exemption from the disease of places in the neighborhood of those affected by it, and between which constant and unrestrained intercourse has been kept up, is another consideration of some importance in favor of non-contagion. The following extract of a letter from the British Consul at Cronstadt, furnishes a strong example of this kind.

‘The small village of Tolbuhin, containing a population of about one hundred and fifty inhabitants, and in daily communication with this place, as it supplies the town with milk and vegetables, has escaped the visitation entirely, and not one being to this day has fallen a sacrifice to the complaint, or had an attack; therefore, to them it has been neither epidemical nor infectious, though their manner of life is not in any way different from that of the inhabitants of this place.’

5. In numerous instances, persons have gone out of infected places and become sick with the disease at a distance, without communicating it to any one else. In the twelfth volume of the London Medico-Chirurgical Transactions, may be found a letter from Mr. Cormick, an English surgeon, dated Tabriz, in Persia, Oct. 1822, mentioning that the Prince of Persia left the city as the disease began to abate, yet from four to six of those who went with him were attacked daily for several days with Cholera, ‘although not a single person of the villages through which they passed, or where they slept, took the disease.’

In the report of Dr. Albers to the Prussian Government, from which we have before quoted, it is stated that ‘during the epidemic, it is certain that about forty thousand inhabitants quitted Moscow, of whom a large number never performed quarantine. Notwithstanding this fact, *no case is on record of the Cholera having been transferred from Moscow to other places,* and

it is equally certain that in *no situation* appointed for quarantine has *any case* of Cholera occurred.

6. Its appearance on board ships at anchor, when there is no Cholera on the neighboring shores, is strong presumptive evidence against contagion. Mr. Nathaniel Grant, late surgeon in the East India Company's service, relates a case of this kind in the *London Medical and Physical Journal*, for October, 1831. It occurred on board the *Sir David Grant*, lying at anchor off Sauger Island, Bengal, in July, 1822, at a time when 'there was no Cholera at Calcutta, nor any where in our neighborhood.' It proved fatal to several of the crew.

7. The great degree of immunity from the disease enjoyed by the attendants on the sick, both in Asia and Europe, can hardly be explained on the doctrine of contagion. Mr. Jameson, in the *Bengal Report*, states, that 'from a medical list consisting of between two hundred and fifty and three hundred individuals, most of whom saw the disease largely, only three persons were attacked, and one death only occurred.'

In the *Madras Report*, it is stated, that out of one hundred and one attendants at the Hospital, of the *Royals*, one only was attacked with the disease.

At *Bombay*, all the attendants of the Hospital escaped, though they were with the sick, by day and night.*

While the disease prevailed at *Orenburg*, two hundred and ninety-nine patients were admitted with it into the military hospital, and not one of the twenty-seven attendants took the disease. Some of the hospital servants were obliged to perform blood-lettings, apply leeches, poultices, and frictions, and administer baths, so that they were compelled to be constantly breathing the exhalations from the bodies and clothes of the sick, as well as to touch and handle them; and yet not one of them had the Cholera. Even the washerwomen of the Hospital escaped. The editors of the *Edinburgh Medical and Surgical Journal*, who are believers in contagion, remark with great candor upon this statement, that 'the immunity enjoyed by the officers and servants of the military hospital of *Orenburg*, is surely sufficient to prove, that *at this period of the epidemic*, the disease could not propagate itself from the sick to the healthy.'

The same immunity has been witnessed at other places, particularly at *Moscow*, as may be seen in *Dr. Albers's Report*. But it is needless to multiply instances of this kind.

* Kennedy, page 57.

What will be the future course of this pestilence, and whether it will probably reach our country, it is impossible to conjecture. Something may be hoped, from the wide-spread ocean over which it must pass, and which may possess the power to disarm it of its virulence; but we should rely with greater confidence on the superior comfort enjoyed by the great mass of our citizens, when compared with those of the other quarters of the globe, the abundance of wholesome food within their reach, the superior convenience and cleanliness of the dwellings of the poor, and generally the absence of what have elsewhere been found the pre-disposing causes. Some consolation, too, may be derived from the fact, that though the number of deaths in proportion to the number attacked has been greater in Europe than in the East, the number attacked in proportion to the whole population has been comparatively small. This is of course owing to the fact, that there is a less number of persons pre-disposed to it there, than in the countries which it visited in Asia, and the number here would no doubt be still less.

We have taken some pains to satisfy our readers of the non-contagious character of Cholera, because we think it a question of great importance, and one which it is very desirable to have correctly settled. We do not allude to quarantines and all the vexatious, expensive and harassing embarrassments, that grow out of them; we waive all considerations of a pecuniary nature, though they are by no means trifling. Restrictions on commerce, infinitely more severe than any that have ever been imposed, would be quietly submitted to, if the disease could be introduced by sea. But we refer to the distress that would be produced, should the Cholera appear among us, if a belief in its contagious character were general.

The sick would be abandoned by all in their hour of distress. So strong is the law of self-preservation in the human breast, that but few, if any, would encounter the danger of administering to the wants of the dying. We fervently hope that the experiment is not to be tried upon us, and that Heaven will avert this calamity from our shores. But should it arrive, we feel confident, that, however the mass of the community may be influenced in their treatment of the sick by the views they have taken of its contagion, the practitioners of the healing art will be true to the sacred cause of science and humanity, to which they have devoted their lives, and that whatever difference there may be in their opinions on some points connected with

the disease, they will all agree in laboring to mitigate its violence. If not operated on by higher motives, they should all bear in mind the immunity of those, who have in other places faithfully watched over the dying, and recollect that the moral courage, which prompts to this, is one of the greatest safeguards against the disease. They should devote themselves without fear to aid and comfort them in the hour of peril; confident, that if their turn come next, it can never come at a better period than when they are engaged with zeal and fidelity in the discharge of their duty.

ART. VII.—*American Colonization Society.*

1. *Fifteenth Report of the American Colonization Society.* Washington. 1832.
2. *Letters on the Colonization Society; with a View of its Probable Results; addressed to the Hon. C. F. MERCER.* By M. CAREY. 1832.

The two pamphlets, of which the titles are given above, will be found to contain a mass of valuable information respecting subjects of great interest to the whole American people, and of immediate importance to more classes than one. Mr. Carey has confined himself almost exclusively to facts. One letter he devotes to the origin of the Society; a second to the state of our colored population; others to the Slave-Trade, the manumission of slaves in this country, the declarations of Legislatures in favor of the Society, and the situation, character, and prospects of the Colony they have founded on the African Coast. It is not our intention to enter at present into the discussion of more than one or two of these subjects at much length; nor need we undertake any thing like a review of either the Report or the Letters. We have named them rather for the purpose of acknowledging our obligations, and of commending them to the attention of all, who are not yet familiar with their contents.

It has been stated, and is perhaps generally understood, that the operations which gave existence to the American Colonization Society are to be mainly attributed to the Rev. Robert Finley, of New Jersey. Such, however, does not appear to